ether, which clears the field a good deal, while acetic acid has very little effect in this way. And with these are a number of loose oil-globules, which have been set free during the manipulation of the tissues. The changes which occur in connection with typhus, as figured by Zenker, are precisely those which I am now speaking of as constituting the various stages of fatty degeneration. (Taf. iii, figs. 1-2; taf. iv, fig. 1.)

Not to lengthen these remarks needlessly, I may yet venture to add, in a few words, to this statement of facts, the conclusions which seem fairly deducible from them.

I believe that the first step towards fatty degeneration of muscular tissue is disintegration of the sarcous elements into filaments or discs, as circumstances may determine. The actual fatty degeneration is secondary to this, and is a consequence—not a cause—of the disorganisation (the functional death) of the fasciculus. In the further changes and ultimate obsolescence of the fasciculus it may play an important part, particularly by resolution of the sarcous elements in the oily matter infiltrated among them through the sarcolemma; but the disorganisation of the part precedes its degeneration.

Inflammation, anæmia, cachexia, want of nutrition, and all the other causes to which fatty degeneration of the heart has been ascribed, have thus much in common that they disorganise the muscular tissue. The disintegration and the fatty degeneration which ensue, may be modified by the abiding influences of the original exciting cause of the disease, but they are transacted by the ordinary agency of the living body quite irrespective of these various possible causes. The actual fatty degeneration depends on the amount of fatty matter in the blood available for this purpose; and it is conceivable that, for want of an available supply of fatty matter, disintegration should not be followed by fatty degeneration at all. Indeed, this seems actually to occur sometimes; the change under certain circumstances does not pass beyond the stage of so-called granular degeneration.

Now, if this be true, a question suggests itself, whether this explanation be limited to fatty degeneration of muscular tissue, or whether it has a wider bearing. For instance, is fatty degeneration of the liver or kidney an infiltration of disorganised cells with oily matter, or a destructive accumulation of oil in healthy cells? Further, is the physiological accumulation of fat in adipose tissue explicable on the same principle, namely, that healthy cell-walls, sarcolemma, or other such tissues are impermeable to fat, and that the accumulation of fat in any part implies that the vitality of the tissue containing the fat has been lowered?

I am not prepared to offer any answer to this question at present. It demands as particular an inquiry into all possible cases of fatty accumulation and degeneration, as I have endeavoured to carry out in respect of the single case of the heart. Beyond all others, the instances of acute fatty degeneration of the brain and liver need careful investigation, more than those where this principle obviously applies. The study of these and such instances is more likely to lead to a just estimate of the value of this principle, than an accumulation of cases where the facility of its application tends to conceal its deficiencies.

TYPHUS IN MALTA. A severe form of typhus fever has lately been very prevalent in several of the country districts in Malta. Its virulence has now abated; and, from a report made by order of the Governor, it appears that it was due to overcrowding in ill-ventilated apartments.

Original Communications.

VENESECTION IN DISEASE.

By W. O. MARKHAM, M.D., F.R.C.P., Physician to St. Mary's Hospital.

THERE is an old and very true saying, that no reputation is better than a bad one; and the saying precisely fits the article venesection, as a remedy in disease. The true estimation of this remedy is, I am satisfied, not admitted, mainly because of the evil reports which attach themselves to the word through its historical connexions. I have no doubt whatever, that if venesection were a modern invention, it would be in much more frequent use than it now is. The remedy thus suffers; and in two ways.

1. The proper uses and right application of venesection are not generally recognised, because the erroneous idea which was attached to its employment in past days is still very much attached to it now; viz., the idea that by its use the supply of aliment to the fire which is raging in and consuming the inflamed structure is cut off.

2. It seems to be still generally assumed, as it was in former days, that the use of venesection is synonymous with the disuse of what are called restoratives; and that those who employ venesection must, of necessity, also at the same time employ low diet, purgatives, and whatever else is comprised under the old idea of antiphlogistic.

So long as these erroneous ideas, which naturally run together in men's minds, prevail; so long as venesection and low diet are regarded as things necessarily connected; so long as venesection is employed with the idea of directly controlling the inflammatory process *in loco* by cutting off the fuel which feeds the flame—so long, in my opinion, will venesection be in bad repute.

My distinguished friend, Professor Bennett of Edinburgh, has not altogether emancipated his mind, as it would seem, from the ideas here referred to. In a very instructive paper lately written by him, On the Treatment of Pneumonia by Restoratives—a paper which appears to have been called forth by a lecture on Venesection by myself lately published in these pages—Professor Bennett gives the result of his long experience in the treatment of pneumonia. He does so to show the good effects of the restorative treatment; and because he observes, "that an effort is being made to restore the dangerous practice of bleeding in pneumonia."

Perhaps, in the lecture referred to, I somehow failed to make my meaning clear. I would, therefore, in explanation, wish to say, that Professor Bennett has quite misinterpreted my meaning; and that I really believe there is *au fond* very little difference of opinion between us as to the practical use of venesection in pneumonia.

In the first place, I have not recommended venesection as a cure for pneumonia. According to my views, venesection has no directly beneficial influence over the local inflammation, and is not used to cure the pneumonia; but solely and wholly to relieve certain accidents which have arisen out of the pneumonia; viz., the congestion of the heart and of those portions of the lungs *which are not the seat of inflammation*. To administer restoratives, therefore, both before, during, and after the venesection, is a perfectly legitimate practice. To suppose that the employment of venesection means the non-employment

of "restoratives"—to regard the two things as antagonistic—is to go back a hundred years for our principles of pathology.

In the sense inferred, therefore, it is quite a fallacy to say, that an "effort is being made to restore bleeding in pneumonia." I do not look upon venesection as of any use *quoad* the pneumonia; nor do I believe it has any directly beneficial influence over any inflammation of the body. I advise its employment in pneumonia for the sole purpose of relieving the patient from the pain and perils of asphyxia—from the dangers of a defective or impeded respiratory action; and, consequently, *only in those cases of pneumonia in which the respiration is seriously interfered with.* But such cases are exceptional ones in pneumonia; exceptional, therefore, also, is the application of venesection in pneumonia.

A consideration of these plain facts will, I am sure, satisfy Professor Bennett, that to compare the employment of venesection, combined with the proper use of restoratives, as here recommended in the treatment of a few severe and exceptional cases of pneumonia, with the employment of venesection, together with blistering, purging, vomiting, and starvation, in all cases of pneumonia—is to bring together in argument things of totally different signification.

Happily, I find proofs in Professor Bennett's own paper, that he himself takes very much a similar view of the matter. He admits that the employment of venesection in certain cases is of service. In fact, he says very much what I myself have said. His words are:

"No doubt, also, small bleedings to the extent of eight or twelve ounces, give relief; but in debilitated persons are dangerous; and in all tend, by weakening the strength at a period when the depressed system is struggling to regain its equilibrium, to prolong the convalescence and favour dangerous sequelæ. Still a small bleeding may be employed as a palliative, with caution, to relieve engorgement of the lungs and congestion of the right side of the heart, although it is very rarely required."

In conclusion, I would just like to ask for the proofs, that there is danger in a moderate loss of blood in any case of pneumonia. Whence has Professor Bennett obtained the practical evidence which has satisfied him of the truth of his assertions of the danger? Assuredly, he has not obtained the evidence from his own observation during the last twenty-five years, as he himself does not employ venesection; and assuredly, also, he can draw no fair or legitimate conclusions as to the dangers of venesections, or as to the true effects of venesection, by judging of it from the results of practice of those who have employed it in conjunction with a starving diet, and calomel, and purging, and blistering.

When Professor Bennett talks of the dangers attending the loss of a few ounces of blood in pneumonia, I cannot help asking him to explain how it is that we daily see so many patients in hospital, surgical and medical, the feeble as well as the strong, losing without apparent injury—and often, and especially in lung- and heart-diseases, to their very great relief—large quantities of blood? What proof do these very numerous facts daily under our eyes afford of the danger of the loss of a few ounces of blood?

And one other question I would put to my friend. Of what real use to us are statistics of the results of treatment of pneumonia, as indicative of the comparative remedial value of bleeding and of restoratives, when the statistics are founded on cases in which the bleeding was employed in conjunction with starvation? Almost all the statistics, drawn especially from French authors, are of this kind, and therefore valueless. Surely, to say that venesection is the cause of the great mortality in pneumonia, in cases where the patients were at the same time both bled and kept upon starvation diet, is a very illogical and inadmissible conclusion.

ON PUERPERAL FEVER.

By T. Snow Beck, M.D.Lond., F.R.S., Member of the Royal College of Physicians, London.

[*Read before the Obstetrical Society of London, February 1st*, 1865.]

[*Concluded from page* 460.]

If the views which have been deduced from this examination of these cases be correct, it follows that one of the principal objects in the prevention of puerperal fever will be to procure a complete and permanent contraction of the uterus after the birth of the child and the expulsion of the placenta.

In the majority of cases of parturition, the uterus contracts readily and firmly; but, in other instances, I must admit that I have found it much more difficult to obtain the complete and persistent contraction than is generally supposed. In our most esteemed works on midwifery, the subject is treated very shortly. For example, the accoucheur is directed to lay his hand on the abdomen, in order that he may satisfy himself that this viscus "is in a safe and proper state of contraction." (Merriman.) Again: "The hand should be placed upon the abdomen to ascertain (from the size of the uterus) whether there are twins; if not, we may proceed to apply the binder, which should embrace the hips inferiorly and the whole abdomen." (Churchill.) Or: "The left hand should be immediately applied over the fundus, in order to maintain a moderate pressure upon the uterus while it is descending towards the pelvis. This should never be neglected; because it ensures a uniform contraction of the uterus, and often the expulsion of the placenta into the vagina." (Murphy.) "In ordinary labour, friction is made with the hand over the hypogastric region: this is repeated from time to time, in order to excite the contractility of the tissue of the organ, its 'degorgement', and the expulsion of any coagula it may contain." (Cazeaux.)

Certainly other means are recommended on the occurrence of hæmorrhage, or in exceptional cases; these means being, to firmly grasp the uterus with one hand, whilst with the other we apply cloths dipped in cold water suddenly to the genitals; to use cold enemata and cold drinks; to pour cold water from a height on the abdomen; to apply pressure by means of a pad placed beneath the binder; to administer ergot of rye; to have recourse to galvanism or electricity; to irritate the organ to contraction by introducing the hand into its cavity; and, lastly, to inject cold water into the uterus. I must, however, admit that in my experience most of these means have failed to effect the object in view. I have not found the external application of cold, cold enemata, cold drinks, or any amount of pressure which could be applied by means of the binder, of much practical good in effecting a firm and persistent contraction of the uterus where there has been any tendency to a lax state. When carefully applied, the binder is, no doubt, a source of much comfort to the woman who has been recently delivered, by supporting the relaxed state of the abdominal walls and giving support to the back. But I have found so little real good from any amount of pressure which could be employed through this means, with the aid of pads or otherwise, whilst the difficulty of applying it and the discomfort it occasioned were so considerable, that I have ceased to employ it for this purpose. When coagula form in the interior of the organ, it is often